Received May 17, 1770.

XXXII. *Experiments on the Blood, with ſome Remarks on its Morbid Appearances*; *by* William Hewſon, *F. R. S.*

Read June 14 & 21. 1770.

AS the following Experiments are made on a ſubject generally thought important, and as the inferences which I have ventured to draw from them ſeem to explain ſome appearances in diſeaſes, they will not, I flatter my ſelf, be thought altogether unworthy the attention of this learned Society.

When freſh blood is received into a baſon, and ſuffered to reſt, in a few minutes it jellies, or coagulates, and ſoon after ſeparates into two parts, diſtinguiſhed by the names of *craſſamentum* and *ſerum*. Theſe two parts differ in their proportions in different conſtitutions: in a ſtrong perſon, the *craſſamentum* is in greater proportion to the *ſerum* than in a weak one; and the ſame difference is found to take place in diſeaſes; thence is deduced the general concluſion, that the leſs the quantity of *ſerum* is in proportion to the *craſſamentum*, bleeding, diluting liquors, and a low diet, are the more neceſſary: whilſt in ſome dropſies and other diſeaſes where the

ſerum

ſerum is in a great, and the *craſſamentum* in a ſmall proportion, bleeding and diluting would be highly improper. As it is therefore ſuppoſed uſeful to attend to the proportions of theſe parts in many diſorders, and even to take indications of cure from them, it has been an object with thoſe who have made experiments on the blood, to determine the circumſtances on which its more perfect ſeparation into theſe two parts depends; it being obvious, that till this be done, our inferences from their proportions will be liable to conſiderable fallacies. Two of the lateſt writers on this ſubject agree, that if the blood, after being taken from a vein, be ſet in a cold place, it will not eaſily ſeparate, and that a moderate warmth is neceſſary: this is a fact that is evinced by daily experience. They likewiſe ſay, that the heat ſhould be leſs than that of the animal, or than 98° of Fahrenheit's thermometer; and that if freſh blood be received into a cup, and that cup put into water heated to 98°, it will not ſeparate; nay, they even ſay, that it will not coagulate; but this, I am perſuaded from experiments, is ill-founded.

Experiment I.

A tin-veſſel containing water, was placed upon a lamp which kept the water in a heat that varied between 100 and 105 degrees. In this water was placed a phial, containing blood that inſtant taken from the arm of a perſon in health; the phial was previouſly warmed, then filled, and corked to exclude air. In the ſame water was placed a tea-cup half full of blood, juſt taken from the ſame perſon;

a third portion of the blood was then received from the ſame vein into a baſon, and was ſet upon a table, the heat of the atmoſphere being at 67°. Now, according to their opinion, the two former ſhould neither have coagulated nor ſeparated, when that in the baſon began to ſeparate; but, on the contrary, they were all three found to coagulate nearly in the ſame time; and thoſe in the warm water, not only did ſeparate as well as the other, but even ſooner.

Experiment II.

The ſame experiment was repeated on the blood of a perſon that laboured under the acute rheumatiſm, whilſt the heat of the atmoſphere was no higher than 55°, and that of the warm water was 108°; and the reſult of this experiment was not only a confirmation of what was obſerved in the firſt, but it even ſhewed, that, that degree of heat was ſo far from leſſening, that it increaſed the diſpoſition to coagulate; for the blood in the cup and in the phial was not only coagulated, but the ſeparation was much advanced before the whole of the blood in the baſon was coagulated. Thence I am led to conclude, that the ſeparation of the blood in a given time, is in proportion as the heat in which it ſtands is nearer to the animal heat, or 98°; or greater in that heat than in any of a leſs degree. And I am confirmed in this inference by experiments hereafter to be related, where the blood in the living animal whilſt at reſt was found both to coagulate and to ſeparate.

It is well known, that the *craſſamentum* conſiſts of two parts, of which one gives it ſolidity, and is by ſome

ſome called the fibrous part of the blood, or the gluten, but by others with more propriety termed the *coagulable lymph*; and of another, which gives the red colour to the blood, and is called the red globules. Theſe two parts can be ſeparated by waſhing the *craſſamentum* in water, the red particles diſſolving in the water, whilſt the coagulable lymph remains ſolid. That it is the coagulable lymph which by its becoming ſolid gives firmneſs to the *craſſamentum*, is proved by agitating freſh blood with a ſtick, ſo as to collect this coagulable lymph on the ſtick, in which caſe the reſt of the blood remains fluid *.

The ſurface of the *craſſamentum*, when not covered with a cruſt, is in general of a more florid red than the blood was when firſt taken from the vein, whilſt its bottom is of a dark colour, or blackiſh. This floridneſs of the ſurface is juſtly attributed by ſome of the more accurate obſervers to the air, with which it is in contact; for, if the *craſſamentum* be inverted, the colours are changed, at leaſt that which is now become the upper ſurface aſſumes a more florid redneſs. This difference of colour, others have endeavoured to explain from the different proportions of

* It may be proper to mention here, that till of late the coagulable lymph has been confounded with the ſerum of the blood, which contains a ſubſtance that is likewiſe coagulable. But in theſe papers, by the *lymph*, is always meant that part of the blood which jellies, or becomes ſolid ſpontaneouſly when blood is received into a baſon, which the coagulable matter that is diſſolved in the ſerum does not; but agrees more with the white of an egg, in remaining fluid when expoſed to the air, and coagulating when expoſed to heat, or when mixed with ardent ſpirits, or ſome other chemical ſubſtances.

the red particles, or globules as they are called, which, ſay they, being in greater proportion at the bottom of the *Craſſamentum*, makes it appear black; but, if inverted, the globules then ſettle from the ſurface which is now uppermoſt, and that becomes redder. But this I think is not probable; for the lymph in the *craſſamentum* is ſo firmly coagulated, as to make it too denſe, to allow of bodies even heavier than the red particles to gravitate through it; for example gold. That air has the power of changing the colour of the blood, has been long known; and the following experiment ſhews it very ſatisfactorily, and hardly leaves room to refer the appearance to another cauſe.

Experiment III.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places; then opening it between two of the ligatures, I let out the blood, and filled this part of the vein with air. After letting it reſt a little till the air ſhould become warm, I took off the ligature, which ſeparated the air from the blood, and then gently mixed them, and I obſerved that the venous blood aſſumed a more florid redneſs, where in contact with the air-bubbles, whilſt at other parts it remained of its natural colour.

There is a difference between the arterial and venous blood in colour; the former is of a florid red like the ſurface of the *Craſſamentum*, the latter is dark or blackiſh like the bottom of the craſſamentum. This change in its colour is produced on the blood as it paſſes through the lungs, as we ſee by opening

opening of living animals *; and as a ſimilar change is produced by air applied to blood out of the body, it is preſumed that the air in the lungs is the immediate cauſe of this change; but how it effects it, is not yet determined.

As the blood is changed to a more florid red in paſſing through the lungs, or from the venous to the arterial ſyſtem, ſo it loſes that colour again in paſſing from the arteries to the veins in the extreme parts, eſpecially when the perſon is in health; but every now and then we obſerve the blood in the veins more florid than is uſual, and it likewiſe frequently happens in blood-letting, that the blood which comes firſt out is blackiſh, but afterwards it becomes more florid: in theſe caſes, the arterial blood paſſes into the veins without undergoing that change which is natural to it.

Some of the neutral ſalts have a ſimilar effect on the colour of the blood to what air has, particularly nitre; thence ſome have attributed the difference of colour in the arterial and venous blood to nitre, which they ſuppoſed was abſorbed from the air whilſt in the lungs. But we know that this is a mere ſuppoſition, for air contains no nitre. Indeed nitre is far from

* That this change is really produced in the lungs, I am perſuaded from experiments, in which I have diſtinctly ſeen the blood of a more florid red in the left auricle, than it was in the right. But ſome authors of the greateſt authority ſay, that they could not obſerve any ſuch difference in a great number of experiments which they made; but this I ſhould attribute to their having been later in opening the left auricle after the collapſing of the lungs than I was; for it ſeems probable, that whatever is the alteration produced on the blood in its circulation through this organ, after it is collapſed, this change cannot take place.

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being the only neutral ſalt which has this effect on the blood, almoſt all the neutral ſalts have the ſame. In making ſome experiments on this ſubject, I have obſerved a much more remarkable effect which neutral ſalts have upon the blood; and that is, being mixed with it when juſt received from the vein, they prevent its coagulation, or keep it fluid, and yet, upon adding water to the mixture, it then jellies or coagulates: thus, if ſix ounces of human blood be received from a vein upon half an ounce of Glauber's ſalt reduced to a powder, and the mixture agitated ſo as to make the ſalt be diſſolved, that blood will not coagulate on being expoſed to the air, as it would have done without the ſalt; and if to this mixture about twice its quantity of water be added, in a few minutes the whole will be jellied or coagulated, and on ſhaking the jelly, the *coagulum* will be broken, and the part ſo coagulated can be now ſeparated as it falls to the bottom, and proves to be the lymph.

In theſe mixtures of the blood with neutral ſalts, the red particles readily ſubſide (eſpecially if human blood be uſed) and the ſurface of the mixture becomes clear and colourleſs; and being poured off from the red part, it is found to contain the coagulable lymph, which can be ſeparated by the addition of water.

I have tried all the neutral ſalts, and have made a table of their effects on the blood, but this table I ſhall not trouble the Society with at preſent: it may be ſufficient to obſerve that in general they agree in producing this change*. And it is leſs neceſſary to

* It may be neceſſary to obſerve here, that thoſe made with the volatile alkali, and with the earth of allum, are to be excepted.

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be particular in giving a detail of their effects, from our not knowing of any uſe this would be in medicine, becauſe we muſt not conclude that their effects in the body would be the ſame as we ſee they are out the body. Indeed, theſe experiments, as well as many others, were not made ſo much with a view of any immediate application to medicine, as to determine the properties of the blood chemically: for, having ſet out with a perſuaſion, that a more particular acquaintance with the properties of this fluid was neceſſary before we could arrive at the knowledge of ſome of the animal functions, ſuch as the manner in which the bile and other ſecreted fluids are formed, I therefore determined to do my utmoſt to throw ſome light on this ſubject. It was with this view that I have made ſome experiments even on living animals, being convinced that ſuch experiments could not otherwiſe be made ſatisfactorily.

When blood is thus kept fluid by neutral ſalts, it ſtill retains its property of being coagulable by heat, and by other ſubſtances as before, air excepted. This method of keeping the blood fluid may therefore be uſeful, by giving us an opportunity of making ſome experiments on the blood, which we could not otherwiſe do, from its coagulating ſo ſoon when taken from the blood-veſſels.

This property of one of the neutral ſalts has been long known, amongſt thoſe who prepare blood for food; for it has long been a practice with ſuch people, to receive blood into a veſſel containing common ſalt, and to agitate it as faſt as it falls, by which means the coagulation is prevented, and the blood remains ſo fluid as to paſs through a cloth, without

leaving any *coagulum* behind: by this means they have an opportunity of mixing it with other ſubſtances for the uſes of the kitchen.

Although the coagulable lymph ſo readily becomes ſolid when expoſed to the air, yet whilſt it circulates it is far from being ſolid: it has indeed been ſuppoſed to be fibrous, even whilſt moving in the blood-veſſels, but erroneouſly.

It is this coagulable lymph which forms the inflammatory cruſt, or *buff* as it is called. It likewiſe forms *polypi* of the heart, and ſometimes fills up the cavities of aneuriſms, and plugs up the extremities of divided arteries. It is ſuppoſed, by its becoming ſolid in the body, to occaſion obſtructions and inflammations; and even mortifications, from the expoſition to cold, have been attributed to its coagulation. In a word, this lymph is ſuppoſed to have ſo great a ſhare in the cauſe of ſeveral diſeaſes, that it would be deſireable to aſcertain what brings on that coagulation, either in the body or out of it.

The blood, when received into a baſon and ſuffered to reſt in the common heat of the atmoſphere, very ſoon jellies or coagulates; the part which now becomes ſolid is the coagulable lymph, as has been ſhewn above. The circumſtances in which it now differs from what it was in the veins, are theſe: it is laid open to the air, to cold, and is at reſt; for whilſt in the body, air is excluded, it is always of a conſiderable warmth, and is always in motion. The queſtion is, to which of theſe circumſtances its coagulation whilſt in the baſon is chiefly owing. This queſtion, I believe, cannot well be anſwered from the experiments that have hitherto been made. It has indeed

indeed been ſaid, that the cold alone coagulated it; for, ſay they, if you receive blood into a baſon, and keep that baſon in warm water, and ſtir the blood well, it can be kept fluid. But in the experiments from which this concluſion was made, I find there has been a deception. In ſhort I have found that it coagulates as ſoon when kept warm, and when agitated, as it does when ſuffered to reſt and to cool. As the ſubject ſeemed to me of importance, I have endeavoured to aſcertain the circumſtance to which this coagulation is owing, by ſeveral of experiments, in each of which the blood was generally expoſed to but one of the ſuſpected cauſes at a time. Thus, in order to ſee whether the blood's coagulation out of the body was owing to its being at reſt, I made the following experiment.

Experiment IV.

Having laid bare the jugular vein of a living dog, I made a ligature upon it in two places, ſo that the blood was at reſt between the ligatures; then covering the vein with the ſkin, to prevent its cooling, I left it in this ſituation. From ſeveral experiments made in this way, I found in general, that after being at reſt for ten minutes, the blood continued fluid; nay, after being at reſt for three hours and a quarter, above two thirds of it was ſtill fluid, though it coagulated afterwards. Now the blood, when taken from a vein of the ſame animal, was completely jellied in about ſeven minutes. The coagulation therefore of the blood in the baſon, and of that which is merely at reſt, are ſo different, that reſt alone can-

not be ſuppoſed to be the cauſe of the blood's coagulation out of the body.

To ſee the effects of cold on the blood, I made this experiment.

Experiment V.

I killed a rabbit, , and cut out one of its jugular veins immediately, proper ligatures being previouſly made upon it; I then threw the vein into a ſolution of ſal ammoniac and ſnow, in which the mercury ſtood at the 14th degree of Fahrenheit's thermometer. As ſoon as the blood was frozen I took the vein out again, and put it into luke-warm water till it thawed and became ſoft; I then opened the vein, received the blood into a tea-cup, and obſerved that it was perfectly fluid, and in a few minutes it jellied or coagulated as blood uſually does. Now, as in this experiment the blood was frozen and thawed again without being coagulated, it is evident that the coagulation of the blood out of the body is not ſolely owing to cold, any more than it is to reſt.

Next, to ſee the effects of air upon the blood, I tried as follows.

Experiment VI.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places, and then opened it between two of the ligatures and emptied that part of its blood. I next blew in warm air into the empty vein, and put another ligature upon it, and letting

ting it reſt till I thought the air had acquired the ſame degree of heat as the blood, I then removed the intermediate ligature, and mixed the air with the blood. The air immediately made the blood florid, where in contact with it, as could be ſeen through the coats of the vein. In a quarter of an hour I opened the vein, and found the blood entirely coagulated: and as the blood could not in this time have been completely coagulated by reſt alone, the air was probably the cauſe of its coagulation.

From comparing theſe experiments, may we not now venture to conclude, that the air is a ſtrong coagulant of the blood, and that it is to this its coagulation when taken from the veins is chiefly owing, and not to cold or to reſt?

It may not be improper to obſerve here, that there are none of the above related experiments I have been obliged to repeat ſo often as the 4th, which was made with a view to determine whether the blood would coagulate by reſt. In the firſt trial which I made, the vein was not opened till the end of three hours and a quarter; and juſt before it was opened I had obſerved through its coats, that the upper part of the blood was tranſparent, owing to the ſeparation of the lymph. On letting out this blood, it ſeemed to me entirely fluid; a part indeed had been loſt, but the greateſt part was collected in the cup, and which afterwards coagulated as blood commonly does when expoſed to the air. From this experiment I imagined that the whole had been fluid; but from others made ſince, I am perſuaded that the part which was loſt had been coagulated; for, from a variety of trials, I now find that though

the whole of the blood is not congealed in this time by reſt alone, yet a part of it is. But as it would be treſpaſſing too much on the Society's time to relate every experiment I have been obliged to make for this purpoſe, I ſhall only mention the general reſult of the whole.

After fixing a dog down to a table and tying up his jugular veins, I have in general found, that on opening the veins, at the end of ten minutes, the blood was ſtill entirely fluid, or without any appearance of the coagulation's beginning *. If opened at the end of fifteen minutes, at firſt ſight it appeared quite fluid; but on a careful examination I have found ſometimes one, and ſometimes two or three ſmall particles about the ſize of a pin's head, which are part of the blood coagulated. When opened later than this period, a larger and larger *coagulum* was obſerved; but ſo very ſlowly does this coagulation proceed, that in an experiment where I had the curioſity to compare more exactly the clotted part with the unclotted, I found, after the vein had been tied two hours and a quarter, that the *coagulum* weighed only two grains; whilſt the reſt of the blood, which was fluid, on being ſuffered to congeal, and then weighed, was found to weigh eleven grains. I can advance nothing farther in this part of my ſubject with preciſion. Nor can I pretend exactly to determine the time at which all the

* I ſay, in general, it was fluid at the end of ten minutes; but I muſt likewiſe mention that in one dog I found two very ſmall particles of beginning coagulation, even at this period; yet in another I could not obſerve any ſuch appearance, even at the end of fifteen minutes.

blood

blood between the ligatures is coagulated. I have indeed opened ſuch a vein at the end of three days, when I found a thin, white *coagulum*, which was a mere film; the *ſerum* and red particles having diſappeared. But the whole is undoubtedly *congealed* long before this period. The manner in which the blood coagulates, when at reſt in the body, has appeared to me curious, and therefore I have taken the more pains to diſcover how it happens, eſpecially as it may aſſiſt us in judging whether or no it coagulates in the heart, ſo as to form thoſe ſubſtances called *polypi*. The abovementioned times will, I believe, be found to be thoſe at which the blood congeals in the veins of healthy dogs: and as I have found, by experiments, that the blood of a dog and of the human ſubject in health jellies out of the body nearly in the ſame time, that is, it begins in three or four minutes, and is completed in ſeven or eight; I ſhould therefore conclude that the blood coagulates nearly at the ſame period in the veins of the human body. But it may be neceſſary to add here, that from experiments which I have made, I have reaſon to believe that the time at which the blood coagulates, is different in different conſtitutions, and in different diſeaſes. For though the blood of a perſon in health is completely coagulated in ſeven minutes after it is taken out of the veins, yet in ſome diſeaſes, I have found the blood fifteen or twenty minutes, nay even an hour and an half, before it was completely jellied.

As we ſee in the above related experiments, that the blood coagulates in the body when ſuffered to reſt for a little time, is it not probable that tis to this cauſe

cauſe its coagulation in thoſe true aneuryſms, which are attended with a pouch, are owing *? For in ſuch enlargements a part of the blood is without motion, which will congeal when at reſt, and in contact with the ſack; and thus one layer may be formed; and the ſack afterwards enlarging, another portion of the blood will then be at reſt; and ſo a ſecond layer may be formed; and thence probably is the origin of thoſe laminated *thrombi* met with in ſuch ſacks.

Likewiſe, to the blood being at reſt, is probably owing its coagulation in the large arteries which are tied after amputation, or other operations; for after moſt of ſuch ligatures there will be a part of the artery impervious, in which the blood can have no motion. The *coagulum* after amputation might indeed be ſuppoſed owing to air; but, conſidering the manner in which arteries are tied whilſt the blood is flowing from them, it does not ſeem probable that the air has any effect on what is above the ligature.

To the blood's being without motion in the cavity of the *uterus*, is its coagulation therein probably owing; hence the origin of thoſe large clots which we ſometimes obſerve to come from this cavity, and which, when they are more condenſed by the ouzing out of the *ſerum*, and of the red particles, aſſume a fleſh-like appearance, and have often been called *moles* or *falſe conceptions*.

In Experiment the 5th, we found that the blood could be frozen and thawed again, without being coagulated: this, likewiſe is an experiment which

* An inſtance of which may be ſeen in the Medical Obſ. and Inq. vol. i. article xxvii. fig. iii.

I have repeated ſeveral times, that I might be ſure of the fact. I have alſo varied the experiment a little, having ſometimes put the vein into a phial of water, and froze the whole in a ſolution of ſal ammoniac in ſnow; and ſometimes I have put the vein into the ſolution itſelf; and three or four times I have put it into oil, and then froze it; but after all theſe trials, the reſult was found to be the ſame. The blood was always evidently fluid on being thawed, and as evidently jellied when expoſed to the air.

Thus far I have related ſuch experiments as I have made, in order to diſcover the cauſes of the coagulation of the blood, out of the body. Next, if agreeable to this learned Society, I ſhall lay before them ſome other experiments that I have made on this fluid.